# MONTHLY NOTICES

OF THE

# ROYAL ASTRONOMICAL SOCIETY.

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Dr. LEE, President, in the Chair.

David Smith, Esq., Birmingham; and
John Robertson, Esq., Glasgow,
were balloted for and duly elected Fellows of the Society.

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## *On the proposed Parallax Observations of Mars in* 1862.

By Prof. C. Piazzi Smyth.

In the important paper of Dr. Winnecke, of Pulkova, in the *Bulletin* of the St. Petersburg Academy, entitled, "Considerations concernant les Observations Méridiennes à faire pendant l'Opposition prochaine de *Mars*, dans le but de déterminer sa Parallaxe," that able astronomer sets forth — and, as we may assume, satisfactorily — first, the large amount of uncertainty at present existing in the received value of the Solar Parallax (probably $\frac{1}{30}$th of the whole); secondly, the extreme desirability of improving this determination; and, thirdly, the unusually favourable opportunity which the opposition of *Mars* offers this autumn, beyond and before most of the recent oppositions, for attaining to the desired consummation. He then proceeds to select a series of stars admirably calculated by their position to be compared in declination with *Mars*, each night from August 20th to November 3d, and, finally, to announce that his method of observation will be bisection, or its equivalent, at Meridian transits with a meridian transit-circle.

Now every one knows that whatever may be done at all with a meridian transit-circle will be done by such an instrument at the Central Observatory at Pulkova, with an accuracy which will be an example to almost every country; but yet one may express surprise that in this case a *meridian*-circle should have been chosen; for the object proposed to be immediately gained is, a micrometrical difference only between *Mars* and the stars of comparison, and is therefore more suitable to a large *Equatoreal*, especially as the number of days during which the parallactic effect will be at its maximum is very small, and observations should rather be accumulated at that particular time than thinly spread over many months of smaller parallax.

Of course the intelligent author of the paper referred to has a reason for the opinion he has come to; and it appears to be, firstly, that in so high a northern latitude as Pulkova, measures taken with an Equatoreal "at great distances from the meridian, as recommended by Mr. Airy, would be attended with very grave inconveniences;" and, secondly, that he has not been certified that there will be any observations of any kind made at any Southern Observatory.

With regard to the first of these reasons, I cannot say anything with authority; but with regard to the second, it appears to me a duty to one of the absent Astronomers of the South referred to (viz. Sir Thomas Maclear, at the Cape of Good Hope), to mention, that I had the honour of receiving from him a letter, towards the end of 1860, detailing his observations of the *Mars* opposition of that year, and describing them with such extraordinary enthusiasm, and looking on them as so essentially his bounden duty, that I at least entertain no reasonable doubt but that he will attend to the opposition of 1862 as honestly and as ably. Furthermore, I should mention —though without any official authority so to do, and subject to correction — that Sir Thomas had made all the observations of 1860 with the 6-inch Equatoreal of the Cape Observatory, and expressed himself perfectly delighted with the superior power of that instrument for that purpose (viz. measuring micrometrical distances between *Mars* and certain stars near him) over the meridian instruments which he had been obliged to employ on former occasions; and he stated his belief that one night with the said Equatoreal was about as good as fifteen nights, I think, with the meridian instruments.

Hence there seems not only a chance amounting to all but certainty, that the *Mars* opposition of 1862 will be observed in the chief Southern Observatory, but that it will be observed with an Equatoreal in preference to a meridian instrument; and with this Equatoreal the observations will be made — according to the plan of 1860 — not so much at great distances from the Meridian, as on and near the Meridian : a position which, it is hoped, will allow the great Pulkova telescope to

be similarly employed, without any of the very grave inconveniences which had been feared for it.

Here terminates, therefore, the first part of the subject; but then immediately forces itself upon our attention this case, whether, granted that the two established Observatories of the North and South fully perform their local parts, is the desired end likely to be obtained — viz. not only an accurate determination of the parallax of *Mars*, but a something so exceedingly accurate that it shall command the attention and elicit the respect of all other astronomers as well, besides those who have assisted at the observations, and to a degree which shall induce them to alter the parallax of the Sun as deduced from the transit of *Venus* in 1769?

In seeking an answer to this requirement, we are met at once by the unflattering circumstance, that oppositions of *Mars* have been observed again and again of late years, and without any such decided step being gained as would warrant an alteration of the received parallax of $8''{\cdot}5776$. What, then, can be particularly said of the opposition of 1862, that should lead us to expect much better results from it than from its predecessors? This is a point which has not escaped the acuteness of Dr. Winnecke; and he accordingly mentions — besides previous observations North and South not being very comparable — that the planet itself was not on those occasions at its least terrestrial distance, which it will be very nearly at in 1862, "or only 0·4 from the Earth, the Earth's mean distance from the Sun being 1·0."

In referring to the *Nautical Almanac*, however, it appears that whereas the parallax of *Mars* will be $21''{\cdot}2$ on Sept. 28th, 1862, it was no less than $21''{\cdot}9$ on July 20th, 1860; consequently the opposition of that year was more favourable in so far by $\frac{1}{30}$, and constituted, therefore, the season when all the observations concerned should have been working for the parallactic difference, rather than in 1862. The Northern Astronomers were, however, hindered in their part of the scheme by their joining the eclipse expedition to Spain — the eclipse occurring on the 18th July, and the *Mars'* opposition observations extending over June, July, and August. This appears to give some excellent hints; for, if many *Mars* observations were lost on that occasion, it has been allowed on the other hand by all persons, that the eclipse gained immensely by the said expedition; and, in truth, expeditions are not only most powerful means for making the most of any special astronomical phenomenon, but they have never yet had full scope given to them; although their results have been, in almost every case when they have been tried, successful. What for instance, it may be asked, would have been the transit of *Venus* in 1796, but for the numerous geographical expeditions that were fitted out to observe it from special and peculiar points of the earth's surface? To this question

we would hardly answer, "Nothing," but rather, "A something so little accurate, that it would have yielded long since to very ordinary *Mars*' opposition observations."

If, then, the *Venus*' observations were, by astronomical expeditions, rendered so much more powerful for their useful end than they otherwise would have been, why should not the *Mars* observations receive the same auxiliary aid? The weaker astronomical phenomenon (for getting at the Solar parallax) surely needs all the more geographical magnifying of the effect, if its result is to be used in correcting the conclusions of the other. But if this magnifying be not done, then, no matter what result the present fixed Observatories arrive at, they will inevitably *not* be considered by the astronomical world to have settled the parallax of *Mars;* for they so plainly leave an opportunity to some one else, only a couple of years after, to make the same observations from a notoriously better base-line.

Had the observations been necessarily to be taken with a meridian circle, and necessarily to have occupied about three months, there might have been some reason for leaving the matter to fixed and established stations; but when they are more naturally to be made with an Equatoreal, as being small differences only, and might all be comprised most profitably within ten days or a fortnight, they come legitimately within the compass of astronomical expeditions; and, viewed in that light, the opposition of 1862 has this decided advantage over that of 1860, that it occurs at an equinoctial season of the year, when the light and heat circumstances of stations in high latitude both North and South will be very nearly similar.

In fact, at that season of the year and for the limited period demanded by equatoreal observations, there would seem to be no reason why an observer should not be sent to the North Cape, latitude $+ 71^\circ 10'$, and another to the southernmost island of Cape Horn, latitude $- 55^\circ 48'$, and thus observe with an intervening difference of $127^\circ$, in place of $94^\circ$ only, the angle between Pulkova and the Cape.

There is little time left now to arrange such expeditions; but we often find, that it is precisely the shortness of time given wherein to perform any work, which calls out men's energies in accomplishing it; and, in such a point of view, there may be still quite time enough. On the understanding, too, that measures on, and near, the meridian, will be made at both stations, rather than at great hour-angles, a very portable folding dome, like Capt. Jacob's, might be easily arranged to allow of limited openings in definite directions, combined with full security to the instruments from inclement weather; and if only a week, indeed only three fine nights, could be secured about the time of maximum parallax, a remarkable micrometrical result might be obtained, and such a one as has not yet been seen in the history of Astronomy; obtained, too, not by anything so costly as what is usually called an expedition,

for the whole thing would rather partake of what the French would term "une *promenade* astronomique."

Even this time, too, might perhaps be shortened, and the result improved, by supplementing the hand-micrometer measures, with photographic pictures of *Mars* and the neighbouring stars, taken in the field of the telescope at intervals through the night on collodion plates, and submitted to screw and microscope measurement on returning to this country.

*Royal Observatory, Edinburgh,*
*June* 12, 1862.

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*Extract of a Letter from M. Winnecke to the Rev. R. Main, dated Pulkowa,* 1862, *May* 30.

"I had the pleasure, a few days ago, to send you a short notice, in which a common plan of action is recommended for the observations of the approaching opposition of *Mars*. You, equally with myself, will be impressed with the importance of the questions at issue; and you will, I hope, at the same time, in looking through my proposals, see that they have not been made without mature consideration of the circumstances. I venture therefore to request that you will use your authority to set on foot, as far as possible, for the approaching opposition, a system of co-operation in relation to meridian observations of declination of *Mars*.

"I add here a few short remarks on some Variable Stars, which you will probably find an opportunity to communicate for the *Monthly Notices*.

"In No. 2, vol. xxii., of the *Monthly Notices*, which I received some weeks ago, Mr. Knott draws attention to the variable star R *Vulpeculæ* = Piazzi, xx. 457. The minimum is, according to his estimations, nearly coincident with a predicted maximum. In Pogson's Ephemeris my name is given as the authority for the time of maximum. I did certainly, two years ago, communicate to Mr. Pogson elements of this star; but I was afraid that an error of transcription had crept into them. My elements, which represent seven maxima, observed in the course of three years, with reference to Piazzi's estimations of magnitude, in August 1803, are namely

Maximum, 1860, Nov. 6 + $138^{d}.6\,n$

($n$ being the number of periods from 1860, Nov. 6);

from which the maxima of 1861 fall on March 25, August 10, and December 27, and those next to be expected should occur on

1862, Sept. 30; 1863, Feb. 18, July 4, Nov. 20.

"I take this opportunity to communicate the following approximate first elements of some other stars, for which no data are given in Pogson's Ephemeris:—

T *Virginis.* 1860·0, $\alpha = 12^h\ 7^m\ 26^s$; $\delta = -5^\circ\ 15'{\cdot}5$.

Variable from the 8th mag. to less than the 13th.

Maximum, 1862, April 12 + $337^d\ n$.

The next maxima, 1863, March 15; 1864, Feb. 15.

T *Ursæ Majoris.* 1860·0, $\alpha = 12^h\ 30^m\ 1^s$; $\delta = +60^\circ\ 15'{\cdot}5$.

Maximum, 1860, Oct. 31 + $257^d\ n$.

Variable from 6·7 mag. to less than the 13th.

The next maxima, 1862, Dec. 11; 1863, August 25.

R *Camelopardali.* 1860·0, $\alpha = 14^h\ 28^m\ 28^s$; $\delta = +84^\circ\ 27'{\cdot}9$.

Maximum, 1861, August 17 + $265^d\ n$.

Variable from 7th mag. to 13th

The next maxima, 1863, Jan. 29, Oct. 21.

S *Cygni.* 1860·0, $\alpha = 20^h\ 2^m\ 34^s$; $\delta = +57^\circ\ 35'{\cdot}0$.

Maximum, 1861, Dec. 24 + $324^d\ n \pm$.

Variable from 9th mag. to less than 13th.

The next maximum, 1862, middle of November.

S *Cephei.* 1860·0, $\alpha = 21^h\ 36^m\ 54^s$; $\delta = +77^\circ\ 59'{\cdot}6$.

Minimum, 1862, Feb. 1 + $470^d\ n \pm$.

Variable from 8·9 mag. to 11·12.

The next minimum, 1863, May.

"The elements of this last star are little to be relied on, on account of the great difficulty of observing the blood-red variables. It appears, like T *Cancri*, to remain very long at its maximum, and to change its light slowly. For S *Leonis* (as the star at $\alpha = 11^h\ 3^m\ 36^s$, $\delta = +6^\circ\ 13'{\cdot}2$, ought to be called, since a star formerly so called is still not proved to be decidedly variable), the following elements will be somewhat more accurate:—

Maximum, 1860, Dec. 24 + $192^d\ n$.

The next maxima, 1862, July 23; 1863, Jan. 31.

*Disappearance of B.A.C. 4923 at the Moon's Dark Limb, 1862, June 9, observed at the Radcliffe Observatory, Oxford.* By the Rev. R. Main.

The star is a double star, the components being of about the 6th and 9th magnitudes. The time of disappearance of each star was instantaneous, and the observations were very good.

| | h | m | s |
|---|---|---|---|
| Sidereal time of disapp. of smaller star ... ... | 15 | 22 | 44·53 |
| ,, ,, larger ,, ... ... | 15 | 23 | 14·03 |
| Oxford mean time of disapp. of smaller star ... | 10 | 10 | 40·03 |
| ,, ,, larger star ... | 10 | 11 | 9·45 |
| Greenwich mean time of disapp. of smaller star ... | 10 | 15 | 42·63 |
| ,, ,, larger star ... | 10 | 16 | 12·05 |

This star deserves the attention of astronomers, as, in addition to its duplicity, it has a large proper motion of $+0^{s}\cdot068$ in R.A., and of $+1''\cdot68$ in N.P.D.

I observed the distance and angle of position of the components of the double star on May 19, 1862; and I found for the distance (mean of six measures) $13''\cdot51$, and for the angle $283^{\circ}\ 51'$.

By comparison with Smyth's measures (*Cycle*, vol. ii. p. 329), it would appear that (unless he is in error) the star has been changing very rapidly both in distance and position-angle.

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*Observations of Jupiter's Satellites and of Saturn's Ring: Occultation of a Star by the Moon.* By C. G. Talmage, Esq.

I send four observations of *Jupiter's* Satellites made at Nice, and three made at Mr. Coventry's Observatory, 5 Tavistock Square, and also an extract from my note-book concerning the peculiar appearance of *Saturn's* ring, about which so much has lately been said.

*Occultation: Disappearance of Jupiter's First Satellite.*

1862, March 17.

| | h | m | s | |
|---|---|---|---|---|
| First contact | 7 | 55 | 4 | Nice M.T. |
| Central bisec. | 7 | 56 | 39 | ,, |
| Total immersion | 7 | 58 | 43·5 | ,, |

*Transit: Egress of Jupiter's First Satellite.*

1862, March 18.

| | h | m | s | |
|---|---|---|---|---|
| First appearance | 7 | 20 | 56 | Nice M.T. |
| Central bisec. | 7 | 25 | 20 | ,, |
| Last contact | 7 | 29 | 49 | ,, |

*Eclipse: Reappearance of Jupiter's Second Satellite.*

1862, March 24.

$11^h\ 55^m\ 53^s{\cdot}74$ Nice M.T.

The time noted is exact; the satellite very distinct and beautifully defined; there appeared a great rupture in the southern belt near the centre, of a pyramidical shape, and a much darker colour than any other part of the belt.

*Eclipse: Reappearance of Jupiter's First Satellite.*

1862, March 24.

$12^h\ 13^m\ 10^s{\cdot}90$ Nice M.T.

The satellite rather faint.

Latitude of Nice $= 43°\ 41'\ 46''$ N.
Longitude ditto $= 29^m\ 7^s$ E.

Nice, February 24, $8^h$, 1862.

The night was not very fine, although *Saturn* appeared very distinct, and the peculiar marking on the ring was well defined, presenting a double serrated edge, with a distinct black line between the upper and lower edges; and the body of *Saturn* had, if I may use the expression, a dirty look. I only mention this, as I have not seen any observations of the kind prior to the above date, which I think shows that we are favoured with a very fine atmosphere, to see that peculiarity in our small glass.

*Occultation: Disappearance of Jupiter's Second Satellite.*

1862, May 2.

$8^h\ 58^m\ 22^s{\cdot}30$ G.M.T.

*Eclipse: Reappearance of Jupiter's First Satellite.*

1862, May 2.

$10^h\ 12^m\ 8^s{\cdot}30$ G.M.T.

*Eclipse: Reappearance of Jupiter's Second Satellite.*

1862, May 20.

$8^h\ 21^m\ 49^s{\cdot}80$ G.M.T.

Latitude of Tavistock Square Observ$^y$. 51° 31′ 30″ N.
Longitude ditto $0^h\ 0^m\ 29^s{\cdot}7$ W.

Mr. Talmage sent also the following observation,

*Occultation: Disappearance of B.A.C. 4923, June 9.*

This is a very fine double star of the 6th and 8th magnitudes. The following are the noted times of disappearance:—

| | h | m | s | |
|---|---|---|---|---|
| Small star, 8th mag. | 10 | 17 | 24·50 | G.M.T. |
| Large star, 6th mag. | 10 | 17 | 52·80 | „ |

At the time of reappearance, a thin cloud hid the small star, but the larger one came out sharp and well defined, instantaneously at

$11^h\ 29^m\ 7^s{\cdot}00$ G.M.T.

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*Results of Meridional Observations of Small Planets, and Phenomena of Jupiter's Satellites; observed at the Royal Observatory, Greenwich, during the month of May,* 1862.

(*Communicated by the Astronomer Royal.*)

*Hebe* (6).

| Mean Solar Time of Observation. | h | m | s | R.A. from Observation. h | m | s | N.P.D. from Observation. ° | ′ | ″ |
|---|---|---|---|---|---|---|---|---|---|
| 1862, May 12 | 13 | 36 | 58·6 | 16 | 59 | 12·55 | 90 | 40 | 15·52 |
| 20 | 12 | 59 | 4·9 | 16 | 52 | 45·10 | 90 | 12 | 48·56 |
| 31 | 12 | 5 | 37·0 | 16 | 42 | 30·58 | 89 | 53 | 9·54 |

*Flora* (8).

| Mean Solar Time of Observation. | h | m | s | R.A. from Observation. h | m | s | N.P.D. from Observation. ° | ′ | ″ |
|---|---|---|---|---|---|---|---|---|---|
| 1862, May 5 | 10 | 38 | 38·6 | 13 | 32 | 47·41 | 89 | 44 | 50·77 |
| 19 | 9 | 33 | 56·7 | 13 | 23 | 6·65 | 89 | 25 | 36·17 |
| 20 | 29 | 29·5 | | 13 | 22 | 35·25 | 89 | 25 | 37·88 |

*Melpomene* (18).

| Mean Solar Time of Observation. | | R.A. from Observation. | N.P.D. from Observation. |
|---|---|---|---|
| | h m s | h m s | ° ′ ″ |
| 1862, May 16 | 11 15 24·6 | 14 53 1·57 | 90 18 6·91 |
| 19 | 11 0 53·2 | 14 50 17·46 | 90 8 1·20 |
| 20 | 10 56 3·7 | 14 49 23·72 | 90 4 58·46 |

*Massilia* (20).

| Mean Solar Time of Observation. | | R.A. from Observation. | N.P.D. from Observation. |
|---|---|---|---|
| | h m s | h m s | ° ′ ″ |
| 1862, May 12 | 12 35 23·6 | 15 57 27·77 | 109 53 30·07 |

*Euterpe* (27).

| Mean Solar Time of Observation. | | R.A. from Observation. | N.P.D. from Observation. |
|---|---|---|---|
| | h m s | h m s | ° ′ ″ |
| 1862, May 5 | 10 15 13·9 | 13 9 18·80 | 94 51 28·60 |
| 20 | 9 9 4·3 | 13 2 6·71 | 94 21 36·86 |

All the observations of N.P.D. have been corrected for Refraction and Parallax.

No Occultations of Stars by the Moon were observed.

*Phenomena of Jupiter's Satellites.*

| Day of Observation. 1862. | Satellite. | Phenomenon. | Mean Solar Time. | Observer. |
|---|---|---|---|---|
| | | | h m s | |
| May 2 | II | Occ. disapp. first cont. | 8 55 2·0 | D. |
| ,, | II | ,, bisection | 8 56 47·0 | D. |
| ,, | II | ,, last cont. | 8 58 47·0 | D. |
| ,, | I | Eclipse, reappearance (*a*) | 10 12 23·3 | D. |
| 17 | I | Egress, bisection | 10 14 48·0 | C. |

(*a*), Very hazy; the observation not very satisfactory.

The initials D. and C. are respectively those of Mr. Dunkin and Mr. Criswick.

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## *Description of a New "Aplanatic" Eye-piece for Telescopes.*

By T. W. Burr, Esq.

Those astronomers who are also microscopists are probably acquainted with an eye-piece for the microscope known as "Kellner's," or the "Orthoscopic," which offers the advantage

of a much larger field of view than the Huyghenian of corresponding power, and with equally good definition. It consists of a double convex crossed lens for field glass (that is, a lens having surfaces of different radii, the most convex side being towards the objective), and a meniscus of great convexity and small concavity for eye-glass. There is no stop in this arrangement.

Having experienced the benefit of this construction with the microscope, I was desirous of applying it to the telescope; and while trying the "Kellner" belonging to my microscope on the telescope of my Equatoreal, with which it produced only a low power, and when about to have another made for the purpose, I was informed that Mr. Thornthwaite (of the firm of Horne and Thornthwaite, Newgate Street,) had improved the "Orthoscopic" microscopic eye-piece, by substituting an *achromatic* plano-convex lens for the meniscus. The eye-glass, in his modification, consists of a double convex crown-glass lens and a plano-concave of flint-glass, forming a combination similar to one of the pairs of an achromatic microscope objective; and this construction (the field-glass remaining a crossed double convex) preserves the advantage of the large and flat field, with better definition and freedom from colour, which has induced the inventor to call it the "Aplanatic" eye-piece.

Having used this form in a microscope, I at once decided to adopt it for my telescope experiments; and therefore requested Mr. Thornthwaite to make me a suitable eye-piece, which, for the purpose of comparison, I required should be of the same power as my third Huyghenian, which, measured by Ramsden's Dynameter, gives with my object-glass a power of 123. The new eye-piece constructed for me, measured in the same way, gives 125 for its power,—a sufficiently close approximation; the slight difference being against the new combination as to size of field.

I have submitted this eye-piece to a careful and lengthened trial, and can now confidently recommend it to observers, as possessing the very great advantage of a much increased field, as compared with the Huyghenian of the same power. This is at once evident on looking at the Sun or Moon, when at least one-third more of the disk of either body is visible with this eye-piece, than with the corresponding Huyghenian. For instance, with my telescope and the ordinary eye-pieces; while a power of 60 includes the whole disk of either Sun or Moon, 80 fails to do so; and with 123 on the Moon, the field includes from the South Pole to about *Plato*, or from the North Pole to near *Bullialdus*, or in longitude, from the eastern border to *Copernicus:* while with the "Aplanatic," of even a little more power, the merest film of one edge is left unseen, and on one favourable occasion, the full Moon was entirely included. The

definition is also quite equal to the Huyghenian at all parts of the field.

The benefit of this eye-piece is also strikingly apparent in viewing clusters, such as the *Pleiades*, *Presepe*, and others; in which small stars are brought up by the increase of power and light, without losing the advantage of a large field; and in the Great Nebula of *Orion*, the effect is very beautiful, allowing the employment of a power which before was disadvantageous, as it made the object dim, and contracted the field; which is now large, and the nebula very brilliant. The division of double stars is rendered easier by this eye-piece, and the range of the telescope among small ones extended. For example, persons who could not see ε *Lyræ* quadrupled with the Huyghenian of 123, had no difficulty in doing so with the Aplanatic, while the detail of the planets, and generally every object, is more readily appreciated.

I send herewith sketches of the construction of the three forms of the eye-piece, viz. the Huyghenian, Kellner's, and the Aplanatic, for comparison; and also place the instruments themselves before you.*

*Highbury, June* 10, 1862.

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## *On the Appearance of Jupiter without a Visible Satellite, Sept.* 1843. By the Rev. W. R. Dawes.

It has been lately suggested to me by the Astronomer Royal, that a remarkable phenomenon, which I happened to observe many years ago, ought to be recorded where it might be readily referred to; and that in fact there could not be a more suitable repository for it than the Royal Astronomical Society. I have much pleasure in acting upon this suggestion, being assured that no other apology can be needed for offering to the Society an observation which was made nearly nineteen years ago. I refer to the appearance of *Jupiter* without a visible satellite in September 1843. The observation of this rare phenomenon was immediately afterwards communicated to *The Times* newspaper in a letter, which principally referred to another Jovian phenomenon, namely, the dark transit of one of his satellites. I supposed at the time that the solitary appearance of *Jupiter* would be extensively observed; and that many communications would be made to the Society by observers possessing the means of more accurately noting the *times* of the

* The sketches and instruments were exhibited at the Meeting of the Society.—Ed.

different occurrences than I happened to have on that occasion. Had I been aware that not a single notice of it would be sent to the Society, I should certainly have made a communication, though in some respects imperfect, in time for the first meeting of the then ensuing session. I mention these circumstances as furnishing the only apology I can offer for having omitted to send some account of the observation to the Society immediately.

On 1843 September 27, in the early part of the evening, *Jupiter* appeared with all his four satellites very near him, and all approaching him. The *fourth* satellite passed on to the disk soon after $8^h\ 30^m$ G.M.T. This I observed at Mr. Bishop's observatory, with his equatoreal refractor of 7 inches aperture, by Dollond. At about $9^h\ 50^m$ I again directed the telescope to the planet, which very soon afterwards passed behind a tree; and as the state of the air was too unfavourable for observations of double stars, in which I was almost entirely occupied, I repaired to my own residence in Park Place, and turned upon the planet a very excellent 43-inch refractor of 2·7 inches aperture by Dollond, power 113.

Soon after $10^h$ G.M.T. (time not exactly noted) the *first* satellite passed on to the disk of *Jupiter*.

At $10^h\ 35^m \pm$ the *second* satellite was occulted.

At $11^h\ 55^m \pm$ the *third* satellite passed on to the disk.

*Jupiter was then without a visible satellite.*

At $12^h\ 30^m \pm$ the *first* satellite passed off the disk.

At $13^h\ 30^m \pm$ the egress of the *fourth* satellite occurred.

It appears, therefore, that for about an hour and twenty minutes the *third* satellite was the only one visible. For about thirty-five minutes *Jupiter* was without any visible satellite. And after the egress of the *first* satellite it was the only visible attendant for rather more than an hour.

The uncertainty of the times specified arises from the fact, that those which were noted at all at the instant of the occurrence, were taken from a pocket-watch, the precise error of which was unknown. And in one instance, the time was estimated from the supposed interval which had elapsed between the occurrence of the phenomenon and the procuring of a light, which was not at hand at the instant. It was this uncertainty which deterred me from presenting to the Society an account of the observation, hoping that others would be presented containing details as to time more accurately noted.

*Hopefield Observatory, Haddenham, Thame, May* 1862.

*On the Periodical Changes in the Belts and Surface of Jupiter.* By W. Huggins, Esq.

In addition to the ceaselessly varying appearances of the surface of the planet *Jupiter*, there would seem to be great periodic changes occurring in the conditions on the planet, of whatever nature those conditions may be, which give rise to the phenomena visible to us. It would seem therefore to be of considerable interest to bring together for careful comparison drawings of *Jupiter*, exhibiting in sufficient detail the more remarkable of the forms which the belts and spots assume at different epochs. For example, the comparatively inactive state of the planet during 1858 and 1859, contrasts in a very marked manner with the strongly defined and actively changing dark belts of the present year. In the hope of exciting attention to these periodical changes of *Jupiter's* surface, and as a humble contribution towards the commencement of such a series of representations of *Jupiter*, I have sent herewith a few drawings, exhibiting in considerable detail, and I believe, with great fidelity, the more characteristic of the appearances of *Jupiter* during the seasons of 1858, 1859, and 1860.* Object-glass 8 inches diameter, and power from 150 to 400.

*Upper Tulse Hill.*

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*Transit of Titan's Shadow.* By S. Gorton, Esq.

Mr. Dawes' account, in the last number of the *Monthly Notices*, of this interesting phenomenon on the 15th of April, concludes by stating that, in his opinion, "from the great size of the shadow, a 5-foot refractor of $3\frac{3}{4}$ to 4 inches aperture would have shown it."

This is completely verified by my observation of the same evening, a short notice of which was read at the meeting of the Society on the 9th May. My telescope is of 4 feet focal length and $3\frac{1}{8}$ in. aperture. I observed with a power of 170, and afterwards with one of 100. At the time I left off observing, the shadow of *Titan* was near the centre of the disk of *Saturn*, and was almost as easy to be seen with the latter power as one of the shadows of *Jupiter's* satellites in transit.

*Downs Road, Clapton, June* 19, 1862.

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* The drawings were exhibited at the Meeting.—Ed.

*On the Appearance of Saturn's Ring*, 1862, *May* 13, $8^h\ 45^m$ *G.M.T.* By W. R. Birt, Esq.

On the evening above specified I had an opportunity of observing *Saturn* at this interesting epoch, with an 8-inch object-glass, which Mr. Slater is now constructing for T. Coventry, Esq., and also with the large refractor of nearly 15 inches aperture.

The state of the atmosphere was not over favourable for so delicate an observation, nevertheless, during fits of good definition, which were not of long duration, I saw very distinctly the fine ansæ broken into minute points or beads of light. It was difficult to get a sufficiently steady interval to count these beads, but the impression I had as to the number of the most prominent was that there were about seven, four on the following and three on the preceding ansa; Mr. Slater noticed five on the following ansa. The following ansa was the longest. On one occasion I obtained a very fine view, when they appeared as *irregularities or projections on the illuminated plane of the ring*, as if such projections were more directly illuminated, and consequently reflected more light than the obliquely illuminated plane of the ring. I did not obtain a confirmation of this interesting appearance. The ring of the shadow formed a somewhat narrow dusky line across the disk, the northern portion being rather the darkest. Mr. Talmage, who was present, saw the fine line of light separating the two, but when I inspected the planet the atmosphere was not sufficiently fine to allow me to witness this interesting phenomenon. Mr. Slater, who observed the beads with a 10-inch object-glass on the 8th of May, at Sheffield, witnessed the phenomena above described, the only exceptions being the number of beads on the following ansa, and the number on the preceding, which he did not estimate.

On the 1st of May Mr. Slater witnessed the transit of the shadow of *Titan* across the disk of the planet.

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*On some Phenomena attending the Disappearance of Saturn's Ring, May 19th*, 1862. By W. Huggins, Esq.

The following notes may perhaps be of some value in support or otherwise of more complete observations of *Saturn*, during a period of so much interest as that through which the planet has just passed.

May 2d. A brief separation of the clouds showed *Saturn* with two bright dots of light upon the eastern ansa of the ring. These were at first taken for satellites. As the haze thickened

and the ansæ faded from view, the contrast between these bright points and the fading ring increased until the ring was lost, when the dots of light alone continued visible as faint and badly defined satellites. As there were no satellites near these positions on that evening, these appearances corresponded probably to the "bright points" seen on former occasions. Vide *Monthly Notices*, vol. x. p. 47.

May 12th. The points of light were again faintly visible, and were best seen when the eye was not directed to them. Passing masses of haze brought them out in strong contrast with the ring, as the pale light of the ansa seemed to suffer greater diminution in proportion than the brighter points.

May 13th. On this evening they were distinctly traced on the eastern ansa to correspond in position with the inner and outer edges of the extremity of the ring. One was seen at the termination of the dark space between the ball and the ring, and the other at the extremity of the visible portion of the ring.

On the 16th the ansa still continued steadily visible, but no indication of these bright points could be seen. The colour of the ansæ, that is, of the surface of the ring seen very obliquely, was a deep bluish purple. A badly defined line of faint shading was suspected across the ball close to the southern edge of the projected ring, in the position in which the shadow of the ring was looked for.

Since that date up to this time, June 10th, no trace whatever of the expected shadow of the ring upon the ball can be detected, though carefully searched for.

On the 17th, 18th, and 19th of May, the ansæ were still seen, though with increasing difficulty, and therefore never totally disappeared.

On the 20th the ansæ appeared of a beautiful dark blue colour, scarcely distinguishable from the dark blue of the sky, which contrasted strongly with the yellow light of the ball.

Upon several occasions, May 18th especially, little roughnesses, resembling miniature satellite-shadows, were suspected on the southern edge of the ring upon the ball, but in every instance these disappeared entirely with the employment of powers from 600 to 950. With these powers, in moments of best vision, the edge of the ring was seen perfectly even and unbroken.

May 17th was cloudy, but at $11^h\ 30^m$ the shadow of *Titan* was seen projecting rather more than half its diameter from the northern edge of the ring.

June 2d. The weather was equally unfavourable, at $10^h\ 45^m$ a brief separation of the clouds showed *Titan's* shadow in contact with the southern edge of the projected ring.

Object-glass 8 inches diameter. Power from 200 to 950.

*June 11, 1862, Upper Tulse Hill.*

*Saturnian Phenomena.* By the Rev. W. R. Dawes.

The night of May 17 was remarkably favourable, especially the earlier part of it, and afforded a very fine view of the expected transit of the shadow of *Titan* across the disk of *Saturn*.

Previously to the ingress of the shadow, I examined the planet most carefully with a power of 620 on my $8\frac{1}{4}$-inch object-glass, which, when twilight had sufficiently faded, was well borne — the features of *Saturn* frequently coming out with beautiful distinctness, and the edge of the disk being sharply defined. The arms of the ring were scarcely at all visible; a very faint gleam of coppery light, at moments of finest vision, being the only indication of its existence beyond the disk of the planet. It has since been seen, on several occasions, much more plainly. On the disk the projected ring appeared as a very dark line a little north of the equator, and of uniform breadth. But I was much surprised that, under the finest definition with this high power, I could discern no trace of the *shadow of the ring*. I expected to see it, if the atmospheric circumstances were sufficiently good, as an *exceedingly fine black line*, stretched across the disk about a quarter of a second to the south of the inner edge of the projected ring; and that the shadow of the satellite would travel almost centrally on the black line — a great part of it, however, falling on the southern portion of the ring. But no such thing was to be found.

Having applied the parallel-wire micrometer with a power of 480, with which the planet was usually very sharply defined; as the expected time of ingress approached, I kept my eye steadily fixed on the eastern extremity of the dark line caused by the projected ring.

At $9^h\ 35^m$ G.M.T., the end of this dark line was observed to be decidedly enlarged, especially on its *northern side* (instead of the *southern*, as was expected).

At $9^h\ 37^m$, the shadow was judged to have just completed its ingress.

At $9^h\ 40^m$, a narrow thread of light was perceived outside the shadow, and on the northern side of the projected ring. It then became obvious that the shadow projected rather more than half its diameter to the *north* of the ring, and so little to the *south* of it that only under the finest views could any projection on that side of it be certainly perceived.

At $9^h\ 52^m$, I carefully estimated that six-tenths of the diameter of the shadow projected from the northern edge of the ring.

After this time the state of the air greatly deteriorated, and I obtained with difficulty some micrometrical measures with powers 375 and 286. The result gave the distance of

the satellite from its shadow in the direction of the axis-major of the ring $=16''{\cdot}06$.

At $12^h\ 10^m$, the shadow was judged to be in the middle of its transit, as determined by the webs of the micrometer.

The difference in the degree of darkness of the ring projected on the ball, and of the shadow of the satellite which fell upon it, was more striking than I expected. The ring looked like a line drawn with a good BB *lead-pencil;* the *shadow* resembled *a spot of ink.*

The position of the shadow with respect to the ring seems conclusively to prove that *Titan*, in that part of his orbit, is somewhat to the *north* of the plane of the ring, with which, therefore, the plane of his orbit is not absolutely coincident. This, indeed, seems to be the case with some of the interior satellites also.

The synodic period of *Titan*, computed from the observed transits of April 15 and May 17, comes out $=15^d{\cdot}967014$, from comparison of the times of complete ingress, and $=15^d{\cdot}966667$ from comparing the times of the shadow's attaining a central position; the mean of the two being $=15^d{\cdot}96684$. The period calculated from the actual orbital motion of the planet, and assuming the sidereal period $=15^d{\cdot}94543$, results $=15^d{\cdot}96967$; the difference amounting to about four minutes.

Nothing, I imagine, can more fully prove the *almost inconceivable thinness of the ring* than the absence of all perceptible shadow. Had it even the least thickness which has ever been ascribed to it (namely, forty miles, by Mr. G. P. Bond, Director of Harvard Observatory, at Cambridge, U. S.), it would be sufficient to produce a total eclipse of the Sun on *Saturn's* equator, as it would subtend an angle more than double that subtended by the disk of the Sun as seen from *Saturn.* This fact seems to me powerfully to support the conjecture which I threw out in my paper on the disappearance of the ring in 1848 (see *Monthly Notices*, vol. x., p. 46), as capable of accounting for the visibility of the ring when its unenlightened surface is turned towards the Earth, namely, *the existence of a pretty damp atmosphere on the rings;* through which the refraction of the light of the Sun, when nearly in the plane of the ring, would throw a pretty strong *twilight* upon them; and this would not only produce a slight degree of copper-coloured illumination upon the surface of the ring, but would also reduce the *shadow* of the ring on the ball to a slight *penumbra;* the *refracted light* from both surfaces of the ring, when the sun is nearly in its plane, being sufficiently strong to render the shadow of so thin a substance, as the ring undoubtedly is, quite undistinguishable.

On May 25 I observed, under sadly unfavourable circumstances, *an immersion of Titan in the shadow of Saturn.* The

state of the air was deplorably bad, and the planet was low at the time of the immersion. The light of the satellite gradually faded, until it was much less than that of *Dione* (then near its eastern elongation), and at last could only be certainly seen during the brief periods of more tranquil vision. The last of these occurred at about $12^h\,30^m$ G.M.T.; and at the next tolerable view, which occurred at $12^h\,33^m$, the satellite had totally disappeared.

Had the planet been near the meridian, and the air in a good state, I believe the observation might have been made with a considerable degree of certainty. It may be hoped that the excellent opportunities which have lately occurred of observing the Saturnian phenomena in America will not have been neglected.

I am not acquainted with any other observation of an eclipse of this satellite; and it is the first which I have been able to see, though constantly on the watch for it near the times of the disappearance of the ring in 1848, and in the present year.

On June 2, the shadow of *Titan* again transited the disk of *Saturn*. The sky was most unfavourable; yet a tolerable view of the planet, at about $8^h\,50^m$ G.M.T. showed the shadow notching the eastern edge; and at $8^h\,55^m$ the shadow had completed its ingress. When it had advanced some way on the disk, I carefully estimated that it projected about as much from the *south* side of the ring, as seen on the planet, as on May 17 it projected from the *north* side of it. Highest useful power, 296. Before the shadow arrived at the middle of its transit, clouds completely covered the sky.

The comparison of the times of ingress, on May 17 and June 2, gives the synodic period $=15^d{\cdot}97500$, being about $1^m\,40^s$ greater than the calculated period.

*Hopefield Observatory, Haddenham, Thame,*
*1862, June 10.*

June 11. The eclipse of *Titan*, last night, was lost through clouds.

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## *On a Solar Spot.* By R. Hodgson, Esq.

The large group of spots which has passed across the Sun's disk during the last fortnight was so peculiar in its formation that upon the 8th inst. I made a micrometrical drawing of it. When it first appeared on the Sun's limb, it was a series of circular and nucleated spots, not quite in contact with each other; as it approached the centre of the Sun, they spread out

until nearly in contact; a day or two after, on the 7th, one of the circular spots appeared to be cut in half and obliterated, having a sharp defined edge; on the 8th, a second circle was similarly cut to a straight line, the other half coalescing with a

spot near it; on the 11th I saw it again for a few minutes, when the whole group was much more detached, and, with the exception of the largest spot, the circular character had entirely passed away.

Telescope, 6 inches aperture. Power, 120.

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At the close of the evening meeting Mr. Carrington explained and illustrated by diagrams the process which he had lately made use of in obtaining the corrections required by his provisionally assumed elements of position of the solar equator. He stated that 86 series of observations alone were found suitable out of about 1000, which his records comprise, it being necessary to reject the greater part on account of the mutual action of one spot on another in its neighbourhood, and to use only isolated nuclei of small extent and regular form. He showed how the required corrections were deducible from the quantities X and Y, found from an equation corresponding to each observation of the form,

$$\delta' - \delta = X \cos \alpha - Y \sin \alpha,$$

in which $\delta'$ is N.P.D., found by use of the provisional elements, $\delta$ the true N.P.D., and $\alpha$ the heliographical longitude of the

spots, reckoned along the solar equator from the ascending node. The values of the elements which he has found from the 86 series are

$$I = 7^\circ\ 15' \qquad \text{and } N = 73^\circ\ 40' \text{ for } 1850{\cdot}0,$$

the values of M. Laugier, deduced about 1841, from 26 series, being,

$$I = 7^\circ\ 9' \qquad \text{and } N = 75^\circ\ 8' \text{ for } 1840,$$

and those of Dr. Böhm from 19 series being

$$I = 6^\circ\ 57' \qquad \text{and } N = 76^\circ\ 46' \text{ for } 1877,$$

The rotation per diem was exhibited in a tabular form for every 5 degrees of declination, as deduced directly from numerous singly determined values in each parallel. The formula

$$865' - 165' \sin^{7}_{4} \text{ lat.}$$

satisfies the whole so nearly, that although it can only temporarily be regarded as an empirical result waiting for theoretical explanation, it is difficult to see how any improvement in it can be effected as a representation of the numbers. Mr. Carrington claims the term in latitude of the solar rotation as one of the chief results of his long-continued observations of the Sun, the whole of which are nearly ready for the press. It would be anticipating the intended future publication to give more than the bare results in the present place.

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In reference to the remarks made in the *Monthly Notices* for February 1862, pages 124 and 125, Mr. Pogson, writing through the Astronomer Royal, desires to make it known that he has undertaken to extend Professor Argelander's noble work southward, by a complete Celestial Survey of the Southern Heavens, as soon as the Variable Star Atlas shall be out of hand. This Survey must, in the first instance, be limited, in its southern direction, by the southern horizon of Madras; but Mr. Pogson looks hopefully to the possibility of ultimately completing it by an expedition to Australia.

Mr. Pogson hopes to begin this work early in 1863; but, he remarks, the limits of time and money, as defined by Professor Argelander's experience, will not apply to a similar work undertaken in an exhausting tropical climate with native assistants.

G. B. Airy.

1862, *June* 17.

## RECENT PUBLICATIONS.

*An Historical Survey of the Astronomy of the Ancients.* By the Right Hon. Sir George Cornewall Lewis. London, 1862.

The history of Astronomy has numerous points of contact with the general history of mankind; and it concerns questions which interest a wider class than professed astronomers, for whose benefit the existing histories have been mainly composed. This remark applies with especial force to the early periods of astronomical observation and science. It has in consequence appeared to the author that an attempt might advantageously be made to treat the history of Ancient Astronomy without exclusive reference to physical science, and without any pretension on his part to a profound and comprehensive knowledge of modern mathematical astronomy. As the accounts of Greek Astronomy, though often meagre and fragmentary, afford in general a firm footing to the historian, and enable him to fix with confidence all the principal stages in its progress, it was considered desirable to begin with this branch of the subject, and afterwards to attempt to determine how far the Greeks derived their astronomical knowledge from other nations, and were learners, instead, as was their custom, of being teachers. And, following this plan, the Astronomy of the Egyptians and Assyrians is first discussed in the latter half of the volume. In support of the alleged antiquity of the astronomical science of these empires, appeal is usually made to their primitive civilisation, and their list of kings, reaching through long lines of consecutive dynasties. It became necessary, therefore, to advert to the early history and chronology of these countries, for the purpose of ascertaining how far they serve to prop up the tottering edifice of Assyrian and Egyptian science. And, as may be conjectured, the conclusion of the inquiry is not in favour of the reliability of these accounts; in particular the author's arguments go to throw doubt upon the possibility of reading and correctly interpreting the Egyptian hieroglyphical inscriptions. The origin of Astronomy is sometimes traced to the Phœnicians, whose commercial wants and nocturnal navigation must, it was supposed, have led to a knowledge of the subject: the concluding chapter of the work relates to the navigation of the Phœnicians, their alleged voyages to Britain and the Baltic for tin and amber, and the supposed circumnavigations of Africa. These subsidiary inquiries form not the least interesting part of the volume.

*Recherches Astronomiques de l'Observatoire d'Utrecht.* Publiées par M. Hoek, Directeur de l'Observatoire et Professeur à l'Université. Première Livraison. 4to. La Haye, 1861. Pp. 1–72.

The work is intended to be a collection of theoretical and practical researches too extensive for an Astronomical Journal, and to be continued at intervals, as the matter presents itself. The present number contains a theoretical investigation of the influence of the motion of the Earth on the fundamental optical phenomena connected with Astronomy—the undulatory theory of light being made use of in the investigation. There are four chapters, relating respectively to Reflexion, Refraction, Aberration, and Astronomical Methods and Instruments; with some additions. One of the conclusions appears to be, that the motion of the Earth produces a sensible modification of the law of refraction.

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*Die Astronomische Strahlenbrechung in ihrer historischen Entwickelung.* Von Dr. C. Bruhns, Astronom der neuen Sternwarte und Professor an der Universität zu Leipzig. 8vo. Leipzig, 1861. Pp. 1–182.

The work relates to Astronomical Refraction, and is divided into two sections—the first containing the history, from the discovery of the phenomenon to the end of the eighteenth century; the second relating to the researches which belong to the present century: the various theories are reproduced, and the mathematical developments exhibited in abridged and simplified forms, with some original investigations. The author proposes to give in a separate treatise the application to observations, and the Tables. There is, at the end of the Preface, a list of the works consulted, which is a convenient *resumé* of the literature of the subject.

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Dr. Bruhns, who is the first Director of the new Observatory at Leipzig, has also published

*Geschichte und Beschreibung der Leipziger Sternwarte, zur Eröffnung der neuen Sternwarte am 8 November 1861, herausgegeben von Dr. C. Bruhns. Mit fünf lithographirten Tafeln.* Leipzig, 1861.

The new Observatory is situate near the town, in a somewhat elevated position in the Johannisthal. The latitude and longitude, as deduced from those of the old Observatory of the

Pleissenburg, are — latitude 51° 20′ 9″·8 N., longitude 49$^{m}$ 33$^{s}$·6 E. from Greenwich. The detailed description of the instruments is reserved until the completion of the great Equatoreal in course of construction by Pistor and Martins, and the publication of the first volume of Observations.

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